two millions sterling. It reminds one of the influence of electric light on gas undertakings some twenty years ago. Gas stock depreciated then many millions. Now they never were so high in value. There was some excuse then. The electric light was a great boon. It was a dangerous rival. There is no excuse now. Nothing whatever about wireless telegraphy has given the smallest indication that it is going to supplant wires. It simply fills a want. Communications between lightships and shore, between ship and ship, between moving ships and shore, between isolated lighthouses and the mainland, become possible. They were not so before.

Messages between the South Foreland and Boulogne have been sent. There is no reason why they should not. Mr. Preece, many years ago, indicated how it could be done. Mr. Marconi has done it in another way. But an isolated experiment, however successful, and a single circuit, though it transmitted messages accurately at the rate of twenty words a minute, is not going to replace one of the present submarine wires, each of which can transmit similar messages at 600 words a minute. Wireless telegraphy may maintain communication with outlying islands when cables break down. It did so by the Post Office with the island of Mull in 1895, before Mr. Marconi was heard of; but it is not going to replace one single cable between Great Britain and the continent. Marconi's system has now been before the public for nearly two years, but we have not heard of anything new from a scientific point of view since it was first published. The last report is that it is possible to direct the signals to one selected point. Two years ago it was said to be able to do the same thing by tuning. The fact is that we have in these repeated sensational experiments a pure scientific apparatus boomed by energetic financial speculators for their own individual gain, and not for the benefit of the public—the worst feature of this money-grubbing age.

THE MICHELSON ECHELON SPECTROSCOPE.

SINCE Prof. Michelson's announcement of his new form of spectroscope, in the *American Journal of Science*, March 1898, and *Astro-Physical Journal*, vol. viii. p. 37, 1898, all having any connection with spectroscopic work have been waiting with great interest to see the performance of the instrument. This curiosity has now been satisfied in a most complete manner, an echelon of fairly large dimensions having been successfully constructed by Mr. A. Hilger, of Islington, which the writer has had the pleasure of examining. Before describing this, it may be useful, for the benefit of many who have not noticed the previous reports, to briefly state the characteristics of the new spectroscope.

In an ordinary diffraction grating, consisting of equidistant lines ruled on a plate of glass or speculum metal, the resolving power is determined by the product ($m\ n$) of the total number of lines (n) and the order of spectrum observed (m). As in this type of grating the succeeding orders after the first decrease in brightness very rapidly, little progress has been made in the endeavour to increase the order observed. Many attempts have, indeed, been made to concentrate the light in one of the first three orders by means of special adjustment of the ruling, so that higher magnifications could be used, but with little or no certainty or equality of result. If, however, instead of obtaining the phase difference by alternations of opaque and transparent spaces, the necessary retardation is brought about by a progressive damping, as it were, of the wave-front by increasing thicknesses of an absorbing transparent material, it would seem that it is possible to throw practically the whole of the transmitted light into any *one* order. In such a case the grating space must be proportionately increased so that it remains commensurate with the degree of the order; and as the lines would need to be made with no more accuracy than before, the grating could be completed in less time, and therefore with less chance of deformity due to temperature and other changes.

Instead, however, of attempting to rule lines on glass or metal, which would be an extremely difficult matter for this purpose, Prof. Michelson took an entirely original step by building up the spaces with a number of strips of glass having optically plane surfaces. The appearance presented by such a dispersive arrangement will then be as shown in the following diagram (Fig. 1), the arrows indicating the directions of the incident and transmitted rays.

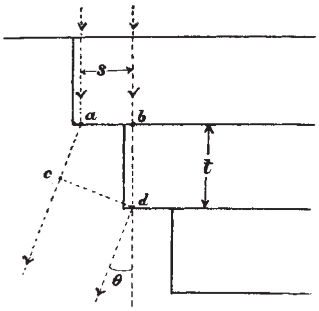

FIG. 1.—Showing paths of direct and diffracted rays through the echelon.

"The interesting feature of the new arrangement is the smallness of the number of elements necessary to give results which may be comparable to those given by the best ordinary gratings. This can be simply shown as follows: Let $ab = s$, and $bd = t$ be the surfaces of one of the steps between two of the parallel plates of the echelon. If then m is the order of spectrum to be observed, we shall have

$$m\lambda = \mu \,.\, bd - ac,$$

or

$$m\lambda = \mu . t - t \cos\theta + s \sin\theta.$$

Therefore

$$\frac{d\theta}{d\lambda} = \frac{m - t\dfrac{d\mu}{d\lambda}}{t\sin\theta + s\cos\theta}$$

and

$$\frac{d\theta_1}{dm} = \frac{\lambda}{t\sin\theta + s\cos\theta};$$

If $\delta\theta$ is the displacement corresponding to $\delta\lambda$, and $\delta\theta_1$ is that corresponding to $\delta m = 1$, then assuming Cauchy's formula,

$$\mu = a + \frac{b}{\lambda^2},$$

and taking as a first approximation

$$m = (\mu - 1)\frac{t}{\lambda},$$

we have

$$\frac{\delta\theta}{\delta\theta_1} = \Big[(\mu - 1) + 2(\mu - a)\Big]\frac{t}{\lambda} \,.\, \frac{\delta\lambda}{\lambda}.$$

"For most specimens of flint glass the coefficient of $\frac{t}{\lambda}$ in the last expression is approximately equal to unity, so that if $\frac{\delta\lambda}{\lambda} = \cdot 001$, say, as in the case of the two yellow sodium lines, and $t = 5$ mm. $= 10{,}000\ \lambda$, then

$$d\theta = 10\, d\theta_1;$$

that is, the two sodium lines would be seen separated by ten times the distance between the successive spectra.

The resolving power of this combination is *mn*, just as in the case of ordinary gratings; and thus, with a battery of *twenty* elements, each 5 mm. thick (corresponding to $m = 5000$), the resolving power would theoretically be 100,000, which is as high as that of the best gratings available at present."

Although, as stated above, the resolving power is independent of the *number* of plates, being determined by the total thickness, yet for any given purpose the number of elements has to be considered. This is evident when it is remembered how closely the successive spectra follow each other. With a small number of plates the overlapping may be so inconvenient as to render the lines unrecognisable, so that, in designing an echelon, this has to be taken into account. If the spectrum to be examined consists of a few sharp lines, the overlapping may be considerable before it causes confusion, and a *few thick* plates will suffice. If, however, the investigation of doubling or widening of special lines in ordinary spectra is needed, then the echelon could advantageously be constructed of a larger number of elements of less thickness to give the necessary resolution.

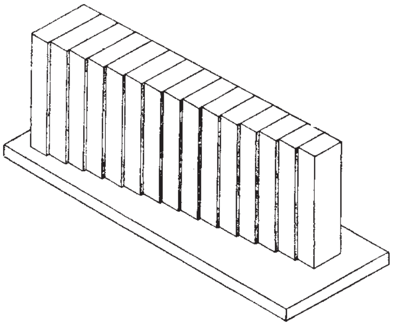

Fig. 2.—The Michelson Echelon. (Scale ⅓.)

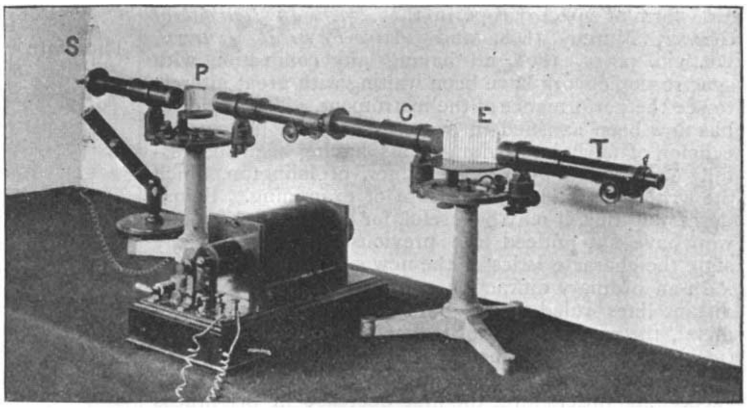

Fig. 3.—The echelon in use.

The echelon made by Mr. Hilger consists of fifteen plates, each 7·5 mm. in thickness and 45 mm. high, the width (s) of each step being 1 mm., and the order of spectrum observed being therefore about the 8000th. Its appearance is shown in the annexed cut (Fig. 2), the cell in which it is usually held having been removed. In use the echelon is laid horizontally on the table of an ordinary spectrometer provided with collimator and telescope, and, as the line of vision is almost direct, it can be placed at once approximately into position. If, however, the slit of the collimator be illuminated with either white light or any source consisting of many radiations, the overlapping of the successive spectra will simply give an almost white band across the field. To obviate this, it is necessary to only illuminate the slit with light which is sensibly monochromatic. In the case of sodium or mercury vapour there need be nothing more than the source itself, as the light is sufficiently simple in their case, but in general some light-filter will have to be used. This may be done by first passing the light through an auxiliary spectroscope, and focussing the spectrum given by it on the slit of the echelon spectrometer, thus isolating a small region which will be approximately monochromatic. This arrangement is illustrated in Fig. 3, which shows how the echelon (E) is fitted up in actual work. The form of apparatus for obtaining monochromatic light will depend on the resources of the observer. Obviously a direct vision prism will be most convenient, as then the source of light can be kept stationary while the spectrum is caused to traverse the slit in order to transmit the different lines. If only an ordinary spectroscope with deviating prism is available, then the light source must be secured to the collimator and moved round with it. There is a very simple method, however, of converting an ordinary prism into a direct-vision one, viz. by fixing a plane mirror to the back of the prism. The arrangement is described by Dr. Fuchs in *Zeitschr. für Instr Kunde*, vol. i. p. 352 (1881), and has later been developed by Wadsworth ("Astronomy and Astro-Physics," vol. xiii. p. 844). In this case, all that is necessary is to revolve the prism-mirror combination about the centre of the back of the prism, the axial emergent ray being always at minimum deviation. This combination is shown in Fig. 3, P being the prism-mirror system between a collimator and telescope; the collimator (C) of the echelon (E) being then directed axially with the telescope of the auxiliary instrument. The observing telescope (T) will be almost directly axial with the collimator, as the deviation seldom amounts to more than half a degree for the line under examination.

With the above echelon the "D" lines of sodium are separated by about sixteen minutes of arc, this being about ten times the dispersion of an ordinary good spectroscope. The successive spectra are only separated by about two minutes of arc, so that generally two orders of spectra are visible in the field together; in practice the echelon can be slightly inclined, thus reducing the intensity of one of these almost to zero, so that measurements can be made on the remaining one with less risk of confusion.

One is surprised at the first experience with the instrument by the brightness of the spectra, considering the enormous dispersion and the path traversed by the light. This is no doubt partly due to the incidence on the plates being very nearly normal, thus diminishing the losses by reflection.

In making the plates it was a very delicate matter to obtain all of exactly the same thickness. This was done by first producing a large plane-parallel plate, and then cutting out the required pieces from it. In the making of this plate the ordinary tests for plane parallelism were found quite inadequate, and every part of it was tested by viewing the interference bands produced between the two surfaces while illuminated by a parallel beam of monochromatic light. Each elementary section of the surface was then refigured until the interference patterns were similar throughout. This will give some idea of the patience and skill required to produce a successful result.

From the fact of its being necessary to use monochromatic light, and also from the limited range owing to the short distance between the spectra, it would appear that the use of the new spectroscope will of necessity be somewhat confined to special branches of research. For the investigation of the Zeeman effect, and the resolution of hitherto undivided multiple lines, it should prove of great service, the relatively bright spectrum obtained rendering it possible to examine the fainter lines which have perforce been neglected up to the present.

Naturally, the first essay in constructing such an entirely new piece of optical work was fraught with many difficulties ; but, encouraged by his success, Mr. Hilger expresses himself as quite prepared to undertake the building up of echelons of much higher power than the one he has just completed. CHARLES P. BUTLER.

FLIGHT OF BIRDS.

"THE way of a bird in the air" has been for ages a perplexing one ; and, until recently, its mechanical explanation has been too much left to persons whom the more sober members of the body scientific were apt to class as "cranks," to use an Americanism. If confirmation of such judgment were necessary, it was afforded by a report, made to the Institute of France about 1830 by Navier, who was no "crank," member and reporter of a commission of eminent scientific men, in which the subject was discussed. Navier drew a distinction, since generally accepted, between hovering, in which the bird remains stationary in the air, as a hawk or kite "winnowing," on the one hand, and progressive flight, accompanied by flapping of the wings, in which the bird moves forward rapidly, as a carrier pigeon or swallow on a journey, on the other. A third mode of flight, called soaring, has, of late, attracted much attention. It consists in the bird maintaining forward motion, straight or circling, sometimes for minutes together, with very occasional and slight flaps of the wing ; in some cases with none at all. In these three kinds of flight, observations easily made by any one show that soaring appears to require the least exertion, though not many birds can manage it well ; progressive flapping flight is common, and many birds can keep it up for hours ; while only a few can manage hovering, which appears to involve much exertion, and, as the Duke of Argyll expresses it, seems to be the most difficult feat of wingmanship a bird can execute. Navier, however, concluded hovering to be far easier than progressive flight, requiring only about one-twentieth of the exertion of this latter ; soaring he does not appear to have considered. His results, moreover, made very heavy demands on the animal, considered as a machine. According to him, a bird must be able to give out energy at, or exceeding, the rate of 1000 foot pounds per second per pound of its own weight, whereas no ordinary land animal whose work is directly measurable gives out continuously for hours more than about half a foot pound per second per pound of its own weight. This excessively disproportionate activity of birds, in the proportion of over two thousand to one, as compared with other animals, seems sufficient to invalidate the basis of calculation ; but the argument is further strengthened on finding that a carrier pigeon, for instance, does not contain enough combustible matter to maintain its own flight, and live, for anything like an hour, if Navier be right. A thousand foot pounds is rather more than one British thermal unit, so that one heat unit, if wholly converted into useful mechanical power, would carry a pound weight for somewhat less than four-fifths of a second. The bird's feathers and bones must be preserved, and water or blood are not combustibles, so that not more than four ounces in the pound of its weight can be reckoned as available combustible matter. If this be as good fuel as paraffin oil (and some birds are oily), each pound of the bird's body can supply about 5000 heat units, able to carry it for 4000 seconds or thereabouts, or less than an hour and ten minutes. At the end of this time the bird would be a wet feathered skeleton with a few ashes inside, rattling among the bones, perhaps. On the other hand, Langley's experiments, which were very carefully made, and agree in all material points with others of the same nature, conclusively show that what is often called an aëroplane—that is, a plane slightly inclined to the direction of its motion in air—can, at reasonable velocities, be made to soar while carrying a load of one pound, by an expenditure of from 5 to 10 foot pounds of work per second. He also shows that the power required to sustain a given weight varies almost exactly inversely as the velocity of flight of the aëroplane ; while Navier, in his calculations, made it vary directly as the cube of the velocity. On all accounts, then, it appears highly probable that the flight of birds is effected, in ordinary cases, by a process much more nearly allied to that of the soaring of Langley's planes than that investigated by Navier. The wings must be taken to act principally as aëroplanes, and the flapping action be used to enable the bird to supply the comparatively small power required for the propulsion of soaring aëroplanes. In confirmation of this view, which the present writer takes to be that now recognised as affording the best explanation of birds' flight, he has shown, in a paper recently read before the Royal Society, that a machine consisting of a vertical cylinder and piston, with a nearly horizontal aëroplane attached to the rod, could, by the reciprocation of the piston, sustain and propel itself in horizontal flight, with an expenditure of energy little exceeding, perhaps falling below, that found requisite in Langley's experiments. In cases comparable to those of birds, rapidity of reciprocation affects the result but slightly, agreeing with the known fact that some birds able to fly long distances flap slowly, while others flap quickly ; whereas this would be improbable if any considerable intrinsic difference in economy of labour existed between slow and quick flapping. As to hovering, the conditions are too far removed from those of experiments on soaring to allow reliance to be placed on numerical results ; there does not appear any absolute necessity for a higher rate of energy-expenditure than in ordinary flight, but the conditions of economical working, particularly as to a sufficiently quick flapping, seem to be more difficult of attainment. In prolonged soaring flight it appears likely, from theoretical investigations by Lord Kelvin, as well as consistent with observation, that local air currents may be largely concerned, as a skilful bird might, by gliding in and out among currents having different velocities, appropriate for his own use and support enough of the extra energy of the quicker currents, to keep himself going continuously, without any exertion, beyond that involved in steering, on his own part.

With respect to the very interesting, though probably not very practical, question of man's mechanical flight, Langley's and Maxim's experiments show it to be at least within the range of possibility ; but, if the conclusions of the paper above alluded to be correct, the most economical method of effecting it would involve using the supporting aëroplanes as birds use wings—that is, as propellers and aëroplanes combined in one. Purely constructive difficulties interfere seriously with designs for movable wings of any considerable size, especially if their motion is a reciprocating one, while there is another whole tribe of difficulties connected with the balancing and steering, very possibly greater than those which beset the beginner on a bicycle ; so that, on the whole, we can scarcely feel as if we were within any very measurable distance of emulating the feats of Dædalus, without almost certainly suffering the fate of his less fortunate son. MAURICE F. FITZGERALD.